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Chinese and Austronesian: what's up?

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0 Introduction

In a series of recent papers, Laurent Sagart (Sagart 1993a,b, 1994, 1995a,b, as well as earlier) has revived the claim that the Chinese languages are genetically related to the Austronesian family (see e.g. Sagart 1993b, n. 1) for references to earlier proponents of this view). The evidence he presents consists mainly of lexical similarities between Proto-Austronesian (PAN, 3000 BC according to Sagart 1994, 4000 BC according to Blust 1995) and Old Chinese (OC, 800–500 BC according to Sagart 1993b, at most 1100 BC according to Sagart 1994), on the basis of which he constructs reportedly regular sound correspondences (Sagart 1993b) that might be taken to explain the origin of Middle Chinese tones (Sagart 1993a). More recently Sagart also claims to have found morphological correspondences as well (Sagart 1994; Baxter & Sagart 1998).

Sagart's early views have been challenged early on by Matisoff (1992), whose cautionary remarks about long-distance comparisons (Matisoff 1990) in general certainly apply to Sagart's claims as well. Matisoff (1990, §2.1) points out that similarities between languages can be due to regional diffusion, calquing, loans, etc., whereas true genetic relationships can be obscured by contamination, blending, folk etymology, etc. His complaints that "Sagart's criteria for phonological correspondence are lax, so that it is easy to find lookalikes in the huge [Austronesian] and Chinese lexica" (Matisoff 1992, p. 159) and that "[h]is criteria for semantic correspondence are also extremely tolerant, and often a prioristic" (*op. cit.*, p. 160) have been noticed by Sagart (1994, in the abstract).

Sagart (1994, abstract) addresses Matisoff's objections and promises "a highly constrained methodology" (see p. 5 below) and a re-evaluation of his own earlier proposals. Since I am not aware of a more recent version of Sagart's proposal, I will mainly review

Sagart 1994.[1] Sagart (1994, p. 275) presents "two kinds of linguistic evidence: morphological congruence [...] and sound correspondences in the basic vocabulary". I will address each kind of evidence below and conclude that neither is conclusive.

Li (1995) objects to Sagart's revised (1994) proposal, coming to much the same conclusions as Matisoff (1992). Although Li (1995) backs up his claims with specific references to the work he is criticizing, many of the specific points he finds unsatisfactory are taken from Sagart 1993b and have been revised in Sagart 1994. The issue of loose semantic cognates (Li 1995, p. 94) has been explicitly addressed by Sagart (1994), as has the level of reconstruction of Austronesian morphology (Li 1995, p. 95). (Sagart (1995a, p. 361ff.) makes the same point in his reply to Li 1995.) Unfortunately Li (1995) does not provide a detailed discussion of why Sagart's claims should not be accepted, but instead concentrates on a lengthy comparison of the Austronesian and Sino-Tibetan core vocabularies, showing that there are few (if any) plausible cognates to be found.

Other critical voices include Pulleyblank (1995) and much more so Blust (1995), sharply contradicted by Starosta (1995). While I am still surprised about the number of people engaged in this debate given the rather weak evidence for Sagart's original claims, I will refer to the valuable points contributed by these researchers in the discussion below. The remainder of this paper is organized similar to Sagart 1994: after a discussion of the morphological data in Section 1, I turn to his lexical comparisons in Section 2, which leads directly to a review of the claimed phonetic correspondences in Section 3. Section 4 tries to caution anyone from jumping to conclusions.

1 Morphological congruence?

Sagart (1994) considers morphological similarity between two (groups of) languages as key evidence for their genetic relatedness. He declares (p. 274) that "[i]t is generally considered that morphology is highly stable and not borrowable [...]. For that reason the diagnostic value of morphological congruence in determining genetic relationships is particularly high." (This view is in and of itself not uncontroversial, but let's believe him for the moment.)

In the case at hand, it might not be totally obvious that there is anything here that can be compared. While there is hardly any controversy about assuming a rich derivational morphology for Proto-Austronesian, it is only recently that the traditional view concerning Old Chinese morphology, which amounted to the belief that Old Chinese was lacking morphology completely, was challenged. Baxter & Sagart (1998) cite a proponent of this

[1] Citations consisting only of a page or section reference will therefore refer to Sagart 1994. Reconstructed forms and glosses are taken from that article as well, unless otherwise noted. Sagart's Old Chinese forms first mention Lǐ Fāngguì's reconstruction (see for example Lǐ 1982) and then in parentheses Baxter's (1992) as modified by Sagart.

belief and then move on to present a fair amount of evidence for a number of infixes and suffixes, whose existence had been assumed e.g. by Baxter himself for some time; for a larger class of prefixes than had previously been postulated; as well as for reduplication processes, whose existence is almost uncontroversial, since they are better reflected in the written texts, whereas the affixes are presumably not represented in the script. If the affixes really existed, it is for this reason that they have been ignored by traditional scholarly work on earlier stages of Chinese, which too often focused exclusively on the writing system and the phonological information that it could record.

I will first review Baxter & Sagart's (1998) arguments for a more elaborate Old Chinese morphology. The morphemes they propose subsume those in Sagart 1994, so any criticism applies to Sagart 1994 as well. Moreover, in some cases where Baxter & Sagart (1998) propose morphemes that had not been assumed previously, they have to modify Baxter's (1992) reconstruction creating structures incompatible with Sagart's (1994) claims. Still, their approach does bring a new quality into the existing efforts of reconstructing Old Chinese that serves both Sagart's and Baxter's agenda, which I take to be the following ones.

Sagart is trying to maximize the structural similarities between Old Chinese, which had occasionally been believed to be lacking any morphological processes at all, and Proto-Austronesian, which must have had a rather complex morphology, given the diverse affixation and reduplication processes found in its descendants.

Baxter on the other hand has for some time been trying to do away with the huge onset clusters that have been posited for the Old Chinese monosyllables: for example, whereas Lǐ (1982) reconstructs **hrjəgw* as the Old Chinese reading of 收 (Mandarin *ṣow*ˇ, 'receive'), Baxter (1992) has **xjiw*. Postulating more morphological processes than had previously been assumed for Old Chinese allowed Baxter to ignore certain irregularities in sound correspondences that had hitherto either prompted the reconstruction of onset clusters or gone unexplained.

For example, Baxter (1992) has 育 **ljuk* 'give birth, rear, raise, bring up, grow up, nourish' and 鞠 **k(r)juk* 'nourish' (glosses from Baxter & Sagart 1998). Baxter & Sagart (1998) however argue for a prefix **k-* that would explain both forms as stemming from a single root **ljuk* with 鞠 **k-ljuk* the derived form.

Although I tend to agree with Baxter's goals of cutting down on implausible consonant clusters in Old Chinese forms, I would only be convinced that postulating a richer morphology is the way to go if Baxter and Sagart were able to provide some content for their morphemes. After all, morphemese are minimal units of meaning, but Baxter & Sagart (1998) seem to treat morphemes rather as units of minimal meaning. About **k-* they say that its "function [...] is difficult to specify with certainty" (*op. cit.*, p. 47), and in the case of 育 ~ 鞠, "the form without **k-* seems general in reference, the form with **k-* more specific" (*op. cit.*, p. 49), which is too unspecific for my taste.

When Sagart (1994, §2) compares Proto-Austronesian and Old Chinese morphology, his analysis suffers from similar deficiencies. For example, he tries to establish a morpheme 'distributed action/object' whose Proto-Austronesian manifestation is the infix *-*ar*- corresponding to Old Chinese *-*r*-. In Old Chinese, for example, there is a verb 合 **gəp (gop)* that Sagart (p. 277) glosses as 'join (two things together)' in one case, and as 'join, unite' a few lines further on. However, a derived form corresponding to the first meaning is 洽 **grəp (g-r-op)* 'unite (more than 2 things)', whereas the second meaning of 合 combines with the infix *-*r*- to form 祫 **gəp (g-r-op)* 'name of a sacrifice to ancestors taken collectively'. What seems questionable about this is whether there really were two words **gəp (gop)* that were (a) homophonous, (b) close in meaning, and (c) both written as 合, yet still distinct, for when each of them combines with *-*r*-, the results are semantically quite distinct. If there was only one word 合 **gəp (gop)*, one of the alleged derived form isn't really derived from it, or is derived via a different morphological process. The words denoting these distributed actions or objects derived by *-*r*- are rather diverse in meaning anyway, perhaps pointing to more varied reasons for their existence. The case for Proto-Austronesian is not very clear in the first place (Blust 1995, p. 286f.), and perhaps Sagart is overly optimistic about having found its remote cousin.

The other infix that Sagart identifies is a nominalizer PAN *-*in*-/OC *-*j*- (p. 278f.). The problem with this is that the Old Chinese derived forms are much more specific than the Austronesian forms. Whereas Chamorro has *faisen* 'to ask' and *if-in-aisen* 'the question', Old Chinese has 火 **hməɾx (hmoj?)* and 燬 **hmjerx (hm-j-oj?)* 'land cleared by fire' where 'land' and 'cleared' are overly specific. Blust (1995, p. 284ff.) provides a detailed description of the meaning and use of PAN *-*in*- and finds only little overlap with the alleged functions of OC *-*j*-. Even Starosta, who views Sagart's proposal much more positively, voices his doubts about the claimed functional similarity between the compared morphemes (Starosta 1995, p. 380f.).

Sagart notes (p. 275) that Austronesian and Old Chinese have in common the place where the infix is inserted into a stem, namely before the first vowel. As Sagart himself point out, PAN *-*in*- has a prefix variant **ni*- (p. 278), which points to a typologically common alternation in infixhood vs. prefixhood that is determined by the shape of the morpheme: vowel-initial infixes can be seen as secondary prefixes, since they attach to a stem as far left as possible but without creating an onsetless syllable. The shared insertion site is thus consistent with typological generalizations (Ultan 1978) and does not necessarily point to a similarity that could only be attributed to a genetic relationship.

Finally, Sagart presents a stative/intransitive verb prefix PAN **ma*-/OC **N*- (p. 279ff.) and devotes a lengthy discussion to justifying the Old Chinese nasal prefix **N*-. On Sagart's interpretation, this prefix would have reflexes in Chinese loanwords in a number of Miao-Yao languages, where it occasionally corresponds to prenasalized stops. Although this argument is presented in much detail, one keeps wondering why it is not taken up or at least referred to by Baxter & Sagart (1998), who are primarily concerned with evidence

for Old Chinese morphology. Blust (1995, p. 287) finds the semantic categories associated with PAN **ma-* and OC **N-* to have little in common. Although Starosta (1995, p, 383) shows how a difference in the reconstructed Austronesian form of the infix can lead to a closer functional similarity with the Old Chinese prefix, this cannot silence Blust's (1995) suspicion that the basis of comparison might have been rather narrow: It is interesting to compare the reconstruction of the Old Chinese prefix **N-* in Baxter & Sagart 1998 with that in Baxter 1992, where one finds **fi-*, following earlier work by Pulleyblank, who joins the debate (Pulleyblank 1995) telling us that he now prefers to reconstruct a 'pharyngeal glide **ă-*' instead. The only concrete evidence for these three different reconstructions is a voicing contrast in the Middle Chinese stops still found today in the Wú 吳 languages. In light of this sparse evidence, Baxter & Sagart's (1998) reconstruction of a nasal prefix seems almost as arbitrary as the other two choices (a nasal could be seen to be less "marked"), but serves Sagart's agenda of making the Old Chinese prefix look more similar to the Proto-Austronesian one, while the alleged similarity amounts to at most one shared feature.

So an obvious shortcoming (quite literally) of Sagart's comparison of morphemes is the shape of the things under comparison: in most cases the observed similarities hold for exactly one segment, in the last case only for one feature of one segment. How much smaller can it get?

2 Evidence from the lexicon?

Sagart (1994, §3) discusses lexical similarities between Old Chinese and Proto-Austronesian extensively and regards it as one kind of evidence for their alleged relatedness. In doing the comparison he claims to have been guided by the following principles.

- "First, strict requirements have been applied to the circumstances of attestation of the lexical material under comparison, as a partial hedge against late innovations." (p. 281)

  On the Chinese side, this means that entries from the post-Hàn rhyme dictionaries, which reflect Middle Chinese phonology, could not be used—or rather: could no longer be used, but had been earlier (Sagart 1993b).

  For Austronesian, this means that only items belonging to the earliest and most inclusive level, Proto-Austronesian, could be used for comparison, but no forms that had only been reconstructed up to, say, Malayo-Polynesian, which again puts a limit on the number of comparable items that is not found in Sagart 1993b.

- "Second, comparisons involving onomatopes and words of expressive origin have not been considered for comparison." (p. 282) Sagart notes that "[t]his excludes 62 of the 231 roots" in Blust 1988, but does not mention that it also gets rid of 25–50 of

the 222 lexical matchings in Sagart 1993b, the exact first number depending on what one considers as onomatopoeic.

- "Third, close semantic matchings have been required. Where semantic shifts are needed, evidence that the shift occurred in other languages [...] is presented." (p. 282)

Sagart concludes his methodological preliminaries with the statement that "[a]s a result of applying these controls, only a small number of comparisons (56 altogether) can be presented here" (p. 282). Of course, 'only' and 'here' only make sense to the reader who is familiar with Sagart's (1993b) earlier list of more than 200 matchings.

Given these provisions, it would seem that Sagart is clearly not guilty of the kind of blind, multi-lateral comparison that Matisoff (1990) and others have been criticizing. However, while following the traditional scholarly approach to lexical comparison Sagart was not always very carful in observing his own guidelines set out before. Moreover, some of his suggestions cannot possibly count as evidence for a genetic relationship. Let me elaborate on this last point, before I return to the issue of how much care Sagart exercised in selecting matching forms.

Occasionally when there is a semantic mismatch between Old Chinese and Proto-Austronesian forms, Sagart cites similar semantic developments found in other languages, as noted in his third methodological remark cited above. Though Sagart never explicitly says that this should be seen as support for the relatedness of imperfect semantic matches, which could be reflexes of a semantic change just like a known change he cites, the very fact that he mentions these similar devolpments makes me think that he attributes some value to it.

He should not.

Something like the following might be regarded as a valid, but weak, argument supporting a particular sound correspondence that Sagart has to postulate: "Proto-Austronesian *-*mp*- corresponds regularly to Old Chinese *p^h-, in which case one might take *-*mp*- to be a direct reflex of a common ancestor and *p^h- as an innovation—in fact, the development of aspirated stops from earlier prenasalized unaspirated stops has been attested in a number of Bantu languages." (Based on, but not quoting, Sagart 1993b, p. 13.) Regular sound change, though not predictable, is based on presumably universal properties of the human physiology and cognitive abilities. If an observed phonetic similarity among one pair of languages constitutes a regular correspondence as a result of sound change starting from a stage of uniformity, then this is at least consistent with the human physiology etc. Thus if an analogous similarity is observed in another pair of languages, for instance Old Chinese and Proto-Austronesian, raising the status of this similarity to that of a regular correspondence cannot be dismissed outright.

For semantic similarities however, pointing out analogous correspondences in a pair of related languages does not lend any more plausibility to the claim that semantically similar items are in fact cognates. Even though semantic notions might be universal (especially if the concepts referred to are), the links among semantic concepts and those between semantic concepts and phonetic forms are not. In particular, if Sagart regards Proto-Austronesian *-*luR* 'to flow' and Old Chinese 水 **hwrjidx (hl-j-uj?)* 'river; body of water; water' (§3.9, p. 285) as cognate forms, it is irrelevant that "[b]oth meanings of 'water' (as substance) and 'river; body of water' are *derivable* from an original meaning 'to flow': cf. IE *wedor 'water', from a root *-wed 'wet; to flow' " (*ibid.*, my emphasis). I do not object to the word for 'water' being *derivable* from the word meaning 'to flow'. However, unless this derivation is actually attested (or inferred indirectly based on solid evidence), this relationship remains a mere possibility. Moreover, the purported fact that a similar semantic correspondence can be found internally in (Proto-)Indo-European would only bear on this issue if semantic change were based on universal human traits and, therefore, largely regular not only within one language, but also across languages.

Similar remarks apply to Proto-Austronesian *q_2*asiN* 'salt' and Old Chinese 辛 **sjin* (*s-j-in*) 'hot-tasting, pungent, bitter', which are claimed to be cognates (§3.22, p. 288). It does not help if Sagart cites scholarly work that relates "Lithuanian *suurus* 'salt' [to] an IE word meaning 'sour' " (*ibid.*, another example follows). The fact remains that 'salt' does not mean the same as 'hot-tasting' or 'bitter'. Exploiting the diversity of the reconstruction sources, one might just as well relate Old Chinese **sjin* 'bitter' to Dempwolff's Proto-Austronesian **pahit* 'to be bitter' (Dempwolff 1938, p. 111), presumably from a hypothetical ancestor **paʃin* and *ʃ > *x > *h together with denasalization in Proto-Austronesian, while in Chinese the first syllable **pa-* of the ancestor form was lost on the way to **ʃin*, which was then reanalyzed as derived from **sin* plus a palatal infix *-*j*-. I do not seriously want to propose such an alternative view, but I do find it quite amazing how easily one can "establish" other "cognate" sets that all in all seem just as plausible as Sagart's, the only difference being that semantic similarity is enforced more strictly at the cost of phonetic similarity. In sum, the task of establishing cognate forms becomes a matter of balancing semantic and phonetic (dis)similarity. Sagart's strategy is to maximize overall phonetic similarity,[2] perhaps because there are less stringent requirements on semantic similarity in cognates.

This last point already illustrates one of the main defects of Sagart 1994, namely the amount of semantic mismatches between allegedly related forms, which goes against Sagart's own third methodological remark (see p. 5 above) requiring "close matchings". This is not to say that there cannot be semantic mismatches between forms that have independently been established as cognates. Strong independent evidence for two semantically different forms being cognates can come from an already established regular sound corre-

[2]cf. his *bon mot* "There is no sound methodological alternative to requiring regular sound correspondences in comparative work" (p. 275)

spondence, but in order to find regular sound correspondences one has to have identified semantically related pairs. However the starting point of such an admittedly circular process should leave as little doubt as possible about what forms are related. Therefore, we would require something close to semantic identity between lexical items from the two languages, only allowing for some inaccuracies in the glosses, in order to establish phonetic correspondences. That done, we might proceed to less clear semantic matches and look for corroborating evidence.

Since Sagart 1994 is at most a starting point (if not a dead end) for a comparison between Old Chinese and Proto-Austronesian, one would want to see very close semantic matches indeed. For my taste, this would specifically exclude pairs like 'neck'/'gullet' (§3.3, p. 284); 'parasitic plant sp[ecies]'/'an edible fern' (§3.16, p. 286f.); 'opposite shore'/'far demonstrative' (§3.19, p. 287); 'salt'/'hot-tasting, pungent, bitter' (§3.22, p. 288); 'grasp in the fist'/'catch' (§3.30, p. 289); 'hack, chop into pieces'/'to cut off'; (§3.44, p. 293); 'insert, stick into a soft surface'/'pierce, prod, stab' (§3.45, p. 293); 'cram, crowd'/'stop up, block' (§3.46, p. 293); 'rice gruel; to mix'/'cooked rice or millet' (§3.53, p. 294f.); 'rice [as food]'/'peeled grain, rice'[3] (§3.54, p. 295); and 'torch'/'fire' (§3.56, p. 295). If we exclude these 11 pairs from Sagart's list, we are down to 45 comparable items.

Let me now return to Sagart's second methodological point (see above), which requires that we discount onomatopoeic and expressive words. A few such items can still be found on Sagart's list. Sagart himself notes that the words for 'to suck', PAN *-*sep*, OC **tsəp (tsi̯ɨ,u̯ɨp)* (§3.48, p. 293f.) and for 'to hammer, pound', PAN **tuqtuq*, OC **təgwx (tuʔ)* (§3.52, p. 294) are quite likely onomatopoeic. In addition to this, I would also consider as onomatopoeic or expressive PAN **u(n)taq* 'to vomit', OC **thagx (thaʔ)* 'to eject from the mouth' (§3.5, p. 284); PAN **tuktuk* 'beak of a bird; to peck', OC **tuk (tok)* 'to peck up' (§3.15, p. 286); PAN **ŋitŋit* '[to] gnaw', OC **ŋiat (ŋet)*[4] 'gnaw, crunch in the teeth' (§3.38, p. 291); and PAN **paqpaq* 'chew', OC **bagh (N+paʔ+s)* 'to chew, have food in the mouth' (§3.40, p. 292), which all refer to parts of the mouth (or the analog of it in other species), or activities involving the mouth (etc.) or throat. There may be arguments about the status of each of this forms, but if we are trying to maximize the quality of the comparison, we would want to exclude the more dubious cases. Excluding all of these pairs from Sagart's list leaves us with only 39 remaining items.

When Sagart notes the "scarcity of cultural items in the [...] list" (p. 295) he is referring to 4 items on his original list, namely those discussed in his §3.53–56, 'rice gruel/

[3]The descendants of Proto-Austronesian **imay* all refer to cooked rice or rice as food, which Sagart mentions in the section title, while the Old Chinese term 米 **midx (mijʔ)* refers to peeled grain, perhaps not necessarily rice. Note that in modern-age Mandarin, which, by the way, draws a clear distinction between rice on the field and rice on the table, 米 *mi̯ɪ* is a derivational suffix referring to all kinds of peeled food, e.g. 花生米 *xwaˀʂɤŋˀmi̯ɪ* 'peeled peanuts' from 花生 *xwaˀʂɤŋˀ* 'peanut'.

[4]Especially since the initial segment is a velar, or – in traditional Chinese terminology – a 'molar' sound 牙音.

cooked rice', 'rice as food', 'house', and 'torch/fire', which he classifies (p. 282) as denoting "cultural notions of great antiquity". However, I excluded 3 of these 4 forms as imperfect semantic matches and find even the remaining one, 'house', questionable, since the Old Chinese word 廡 **mjagx (m-j-a?)* seems rather rare for something referring to an important cultural concept. However, Sagart's first methodological principle (see above) would require him "to exclude rare words" (p. 281).

Sagart instantly dismisses the absence of cultural concepts from his list as "not necessarily meaningful" (p. 295) and notes that "a majority of items in the list are not of a kind that would normally lend itself to borrowing in a limited contact situation" (p. 295f.). He also takes it to be significant that most words on his list are verbs, "a word class believed to resist borrowing well" (p. 296) as he claims. Sagart effectively forestalls certain complaints that personal pronouns and numerals are completely absent from his list when he declares that "[t]his is not abnormal in the East Asian context" (*ibid.*). Sagart correctly points out that addressing people directly is traditionally avoided in the macro-culture of this area, and personal pronouns have therefore often been replaced by hedging expressions. Moreover, there are a number of cases where numerals have been borrowed. However, the last argument can be turned against Sagart. If we had been thinking that numerals were resistant to borrowing but are shown cases where numerals might actually have been borrowed in the East Asian area (Sagart 1995b, p. 202ff.), why should we still believe that verbs are any more resistant?

3 Sound correspondences?

Sagart (1994, §4) includes a quick summary of the sound correspondences that hold between the Proto-Austronesian form and the Old Chinese form in each item on his list. The most striking aspect of this comparison is that Proto-Austronesian words are in general polysyllabic, whereas Old Chinese words are almost exclusively monosyllabic (at least all forms on Sagart's list are). He concludes that "[t]he ancestor language ("Proto-Sino-Austronesian" [...]) is assumed to have been polysyllabic [...]. The shift to Chinese monosyllabism occurred through the loss of nonfinal syllables" (p. 296). As an example for a similar development, he cites Huihui, a Chamic language spoken on Hǎinán 海南 island off the Southern cost of China, which is developing into a monosyllabic tone-language, presumably under regional influence from Chinese, from an earlier non-tonal, polysyllabic stage (more examples are provided in Sagart 1993a).

What this claim leaves unexplained is the alleged presence of remnants of Austronesian infixes in Old Chinese words. This was pointed out by Blust (1995, p. 286). He explains that PAN **-in-* attaches after the first consonant of a stem, so that one finds morphologically complex words of the shape *C-*in*-VCVC in Proto-Austronesian. But if all syllables except the last one are lost on the way to Old Chinese, how did the infix survive?

The material that can be compared phonetically thus has the shape of Proto-Austronesian (final) syllables: CV(C). This is partly because "affixes, whether OC or PAn, have been disregarded" (p. 296). For the Old Chinese forms, disregarding affixes often implies getting rid of initial consonant clusters by identifying certain segments as (reflexes of) prefixes or infixes. For example, when comparing PAN **paqpaq* '[to] chew' with OC **bagh (N+pa?+s)* 'to chew, have food in the mouth' (§3.40, p. 292), Sagart takes the final syllable **-paq* of the PAN word and compares it to the OC stem **pa?*. He has thus identified the sound correspondence $*q \sim *?$ (p. 297), apart from the trivial correspondences.

Although Sagart does not quite engage in megalocomparison, parts of Matisoff's (1990) criticism of ill-founded comparative practice do apply in Sagart's case as well. In particular, Matisoff (1990, §4.1) criticizes Paul Benedict's conception of the development of the Tai-Kadai and Hmong-Mien branches from Proto-Austro-Tai: under areal influence of Chinese, the languages of these two branches have reduced the complex polysyllabic morphemes of the proto-language to tone-bearing monosyllables.[5] Given this disparity between the rich proto-forms and its impoverished descendants, Matisoff concludes (*op. cit.*, p. 116), "the etymological possibilities are endless."

Clearly, the same can be said about Sagart. Once the basis for comparison has been narrowed down to two or three segments, it is not surprising that he has found what he was looking for, especially since phonetic matches do not have to be perfect as long as the later forms are endpoints of a "natural" sound change leading away from a common ancestor.

The "adjustments due to change in the OC transcription system" and the "modifications and innovations, which will be fully justified elsewhere [probably Baxter & Sagart 1998]" (p. 296) are not as minor as Sagart makes them appear. Identifying a more elaborate Old Chinese morphology (as mentioned above) will of course result in a re-evaluation of the Old Chinese reconstruction efforts. Moreover, Sagart not only made some adjustments in his transcription system, he in effect switched from Lǐ Fāngguì's reconstruction, on which Sagart 1993b was based, to a modified version of Baxter's (1992) system that incorporates the richer morphology described in Baxter & Sagart 1998. Although most of the sound correspondences that Sagart (1993b) had originally proposed could be maintained, the proposed correspondence PAN **-s* $\sim$ OC **-h* is now no longer supported by Sagart's (1994) data, as he himself notes (p. 298). However, that correspondence had played a crucial role in Sagart 1993a, §2.1.3, where it was taken to explain the later development of the falling tone in Middle Chinese.

Recall that Baxter & Sagart (1998) proposed a prefix **k-* for Old Chinese, which I found somewhat dubious above (see p. 3). But even if we believe Baxter & Sagart (1998), their strategy may backfire, at least as far as Sagart's agenda are concerned. For instance, whereas Baxter (1992) reconstructs the Old Chinese reading of 結 as **kit*, Baxter & Sagart

[5]Incidentally, these examples are mentioned in Sagart 1993a in his discussion of monosyllabic reduction and tonogenesis.

(1998, p. 57) have **k-lit*, probably because of other characters with the phonetic component 吉, such as 臺 **lɨ* (Baxter's reconstruction). But Sagart (1994) wants to relate 結 'to tie, knot' to Proto-Austronesian **SikeC* 'tie, attach to' (§3.50, p. 294), cf. also his earlier view (Sagart 1993b, p. 40) that Old Chinese 結 **kit* is related to Dempwolff's Proto-Austronesian **Ra(ŋ)kit* 'tie together, raft' and Blust's root **-kit* 'join along the length'. Since in other cases, Sagart disregards affixes in his comparisons, does this mean that in the present case we should ignore Old Chinese **k-lit* because it is morpholgically complex and only let its stem **lit* enter into the comparison? If so, this pair involving the word for 'to tie' is off the list too.

While trying to make the morphological congruence between Proto-Austronesian and Old Chinese more plausible, Sagart postulated a richer morphological system for Old Chinese. However, that sometimes has lead him to identify different stems for which the comparison with Proto-Austronesian suddenly breaks down. Sagart is apparently attempting a balancing act that makes his claims about morphology more plausible while trying to keep the lexical comparisons stable. But if Sagart's views on the reconstruction of Old Chinese phonology and morphology can and do change so easily—not that there's anything wrong with that—I would expect that he would be more cautious with his claims about Proto-Sino-Austronesian phonology. But the confidence with which Sagart has been advocating the Sino-Austronesian connection, while freely replacing the Old Chinese reconstructions he used with others, portray him to me as someone who thinks he knows what the truth is before he has found it.

How can we be sure that Sagart won't change his views in the future?

4 Conclusions?

We can't.

Sagart (1994) ended his discussion by admitting that the Tibeto-Burman languages "may stand closer to Chinese (and to PAn) than I had originally assessed" (p. 303). In Baxter & Sagart 1998, n. 23 he has fallen back completely to the more traditional view that "Chinese and Tibeto-Burman are closer to each other than either is to Austronesian".

And rightfully so, since the evidence for Sino-Austronesian is still rather weak, to say the least. Even if we accept Sagart's entire list of matching words, we cannot be fully certain that the similarity is due to a genetic relationship and not to borrowing, especially since it is known that the Austronesians began their conquest of the Pacific islands from the Chinese coastal area and were therefore most likely in contact with the ancestors of the Chinese peoples. (See Matisoff 1992, p. 159 for the same argument.)

It is hardly worth reiterating that it cannot be proven that any two languages are unrelated. Unless we are presented good enough evidence to the contrary, we should assume

that Old Chinese and Proto-Austronesian are unrelated. Sagart's explanations for the absence of personal pronouns or numerals in his list (see p. 9 above) are therefore unnecessary, since the absence of anything would only need to be explained once there is compelling evidence in favor of a genetic relationship.

But there isn't.

There is at most evidence for maximally three relatable segments in Proto-Austronesian/Old Chinese word pairs in 38 (on my count) to 56 (on Sagart's (1994) count) cases, which should be subject to further scrutiny. I would think that chance (see Blust 1995, p. 286) and/or borrowing (see Li 1995, p. 95) stand a good chance of being borrowed as alternative explanations.

Unfortunately, Sagart (1994, §7) comes pretty close to "megalomania" when he considers the possiblity of an "extended Austric superfamily" (p. 301). This is based on recent proposals for an Austric macro-family that would subsume Austronesian and Austro-Asiatic. But Sagart does not stop here, he also considers Tibeto-Burman as a likely candidate for membership (only to claim later (Sagart 1995b) that Chinese is more closely related to Austronesian than to Tibeto-Burman). Interestingly, in his assessment of the data used as evidence for a Chinese/Tibeto-Burman connection, Sagart (1994, p. 301f.) explains that the morphology shared between Chinese and Tibeto-Burman is "is limited, and the "basicness" of some of the [shared] lexical items, though suggestive, is no fool-proof guarantee against borrowing in an intimate, long-term contact situation [...]. The problem is compounded by poorly understood sound correspondences, which make it hazardous to distinguish between cognates, look-alikes, and loanwords." These are profound insights that seem to be completely absent from the earlier parts of his paper.

With so much proposed lumping going on already (see the discussion in Blust 1995, p. 291f.), it seems to be merely a matter of time until someone proposes an even bigger Asia-Pacific group that will include Sinitic, Austric, Tibeto-Burman, and in addition Miao-Yao (cf. Sagart 1994, p. 303), Tai and Hmong-Mien (Matisoff 1992, p. 159 mentions Paul Benedict's Austro-Tai hypothesis), Japanese (cf. Matisoff 1990, p. 115) on a proposed relationship between Japanese and Austronesian), Na-Dene (cf. Matisoff 1990, p. 118 on a proposal originally due to Sapir that relates Sino-Tibetan to Na-Dene), or even Indo-European (*ibid.*, citing Edwin Pullleyblank's hypothesis) as well. Next, one may believe that "genetic relationship is plainly transitive" (Greenberg's view, expressed in his *Language in the Americas*, cited in Matisoff 1990, p. 114) and obviously symmetric, and Sagart (1994, p. 300) apparently does subscribe to this when he says that "any relationship that is valid of either OC or PAn is valid of both". In this case one might add more languages and language groups based on a single hypothesis from the literature (e.g. Tamil, once claimed to be related to Japanese by Ohno, and hence Dravidian), which would ultimately lead to Proto-World. No-one who is sympathetic to this idea has to look very far to find forms that might make membership of Sino-Austronesian in Proto-World plausible: Sagart (1994,

§3.3, p. 284) has Proto-Austronesian **l[i]qeR* 'neck' and (hence??) Old Chinese 咽 **?in* 'gullet', which are of course relatable to Greenberg and Ruhlen's Proto-World **maliq'a* 'neck, to swallow'.

But then we might ask (to end on a low tone): so what.

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